effusions, œdematous and ascitic fluids, are capable of being absorbed by the organism.

His experiments had unquestionably shown that gelatin injections augmented the coagulability of the blood, and that the quantity of gelatin in the peritoneal cavity several hours after the injection was considerably less than the amount originally introduced. From this it must be concluded that some of the gelatin was absorbed. He held that the opinion advanced by Laborde, that peptonization of the gelatin was necessary before absorption could take place, was purely hypothetical.

Laborde, in reply, stated that the results of recent experiments had confirmed his earlier ones in that he found that practically no gelatin was absorbed after its injection into the peritoneal cavity. He held, however, that at the end of a certain time it may disappear, owing to its undergoing peptonization and absorption. He believed that any action the gelatin possessed was due to its conversion into peptone. Laborde stated that the determination of the coagulability of the blood presented the greatest difficulties, because it varied markedly from one moment to another without any regularity, and was so readily influenced by faulty technique. He believed, then, that no precise conclusions could be drawn from Lancereaux's experiments on the coagulability of the blood.

Hayem took part in the discussion, and stated that, although he had had a great deal of experience in the determination of the coagulability of the blood, his results had been so variable and contradictory that he was forced to lay them aside. He did not agree with the view of Laborde that for a substance to be absorbed it must be dializable. In support of his view he cited the observation that if dogs' blood be injected into the peritoneal cavity of the goat the red cells of the former, which are larger than those of the latter, are found in the circulating blood of the goat after four or five hours.

Laborde objected to Hayem's argument, because in this case one has to deal with a living body, the red blood-corpuscles; whereas gelatin is an unorganized and non-dializable substance.

[During the past three months the injections have been used in five cases of aneurism in my wards. In one the first injection caused so much pain that the patient refused further treatment. One patient died (after about five injections) of hemorrhage. Of the remaining three cases in which the injections have been given systematically one is decidedly better; two present no changes.—W. O.]

The Effect of Turpentine Inhalations in Experimental Tuberculosis.—J. Héricourt and Charles Richet (*Société de Biologie*, November 18, 1898) at the session of the Société de Biologie on November 12, 1898, gave the results of their experience with the use of turpentine inhalations in the treatment of experimental tuberculosis in dogs. The administration of the drug by inhalation is preferable to that by subcutaneous and intravenous injection or to administration by the mouth, for various reasons. It does not require an operation, the drug is brought into direct contact with the bacilli in the lungs, and the dose can be carefully graduated until nearly a poisonous dose is reached.

Their procedure was to place the dogs in a box through which air charged with turpentine vapor was passed. They were kept in the box for one hour, and during this time 1200 litres of air charged with 12 grammes of turpentine were passed through it. The inhalations were given every fourth day from March 14th to August 2d.

In order to compare this method of treatment with some others they inoculated eleven dogs on March 2d with a tenth of a cubic centimetre of a three-months' culture of human tuberculosis. Three dogs were not treated, and they died on the twentieth, twenty-seventh, and forty-ninth days after inoculation, respectively. Three others were treated by intratracheal injections of iodine (0.015 gramme to the litre), and two died on the twenty-fourth and sixty-sixth days. With two of the dogs intravenous injections of turpentine were used, and they died on the forty-fifth and eighty-fourth days. The turpentine inhalations were used in the remaining three dogs, and only one died, death occurring on the twenty-fourth day. The other two dogs were alive and healthy at the time their report was made, two hundred and fifty-one days after inoculation. The turpentine inhalations had thus given by far the best results. They claim that the success of this treatment is emphasized by the fact that in not a single instance out of two hundred dogs inoculated during a period of ten years and allowed to go untreated had the animal lived longer than six months.

The series of animals in which they had tested this treatment is unfortunately too small to draw definite conclusions from. The practical importance of their work rests in the possibility that similar turpentine inhalations may also be of value in the treatment of human tuberculosis.

The Inoculation Theory of Malarial Infection.—BIGNAMI (*The Lancet*, December 3 and 10, 1898), in a most interesting article on the mode of conveyance of malarial infections, refers to a previous communication in the same journal for 1896, in which he held that it was very improbable that malarial infections were conveyed either by water or air. In the same article he expressed the opinion that "malaria behaves itself with regard to man as if the malarial germs were inoculated by mosquitoes." Great stress was attached to one argument in favor of the inoculation theory, viz., its analogy to the Texas cattle fever, in which Smith and Kilborne had demonstrated that the infection was conveyed by means of the tick. Bignami then refers to Manson's discovery of malarial flagellæ in the bodies of mosquitoes and to his view that these bodies represented the extracorporeal life-cycle of the malarial parasite. He cites the important discovery of Ross that the gray mosquito is capable of transmitting the halteridium-Labbé from sparrows whose blood contained this organism to uninfected birds.

In 1894 Bignami and Dionisi conducted some unsuccessful experiments with the view of determining whether the mosquito was capable of conveying the disease. Bignami does not think that the negative results discredit the hypothesis, as he believes that the proper species of mosquito was not used.

Laveran, in the last edition of his work on malaria, merely alludes to the inoculation theory. Koch, while not bringing forward any proof in support of the view, recently expressed the opinion that the malarial parasite has the same relation to the mosquito that the parasites of Texas fever have to the cattle-tick.